

(yf ELT)

Fred, E.B.

November 22, 1976

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Dr. E.B. Fred
President Emeritus
University of Wisconsin
Madison, Wisconsin 53706

Dear Dr. Fred,

One small way that I can start to repay my own debt to Edward L. Tatum is to undertake the task of the memoir scheduled for the series of the National Academy of Sciences. I put together a circular that I sent to a number of other mutual friends, and I hope you do not mind looking at that as background to the particular request that I would like to put to you. I have been trying to get a clearer picture of Ed's development as a scientist and since this was over 40 years ago, it is not so easy to find reliable documentation or recall. I am sure you will understand why I single you out for a unique competence although I realize that you have had such a busy life since then that you might or might not be in a position to afford much rich detail.

I guess it was my particular hope that you might still be able to retrieve papers and letters concerning Ed Tatum's career as a graduate student and post-doctoral fellow - these would be of unique value for the archives that I am trying to collect. In addition, needless to say, any personal recollections that you might wish to have, would be treasured additions to this memoir.

I first met Ed myself in 1945 through our mutual friends and colleague, Francis J. Ryan, at Columbia University. As you may well recall, I then joined him at Yale University for a short while before coming to Wisconsin myself in 1947.

It is the period before 1946 where I have to work hard to get biographical material, and in particular as concerns his earlier life as a graduate student and up through the time that he began to work at Stanford in 1937.

Some of the particular questions that are in my mind at the present time are: (1) exactly how he came upon his choice of microbiological chemistry for his career; (2) how you and Peterson found him as a student, fellow, and colleague, and the particular role that he played in your joint investigations; (3) how he came to choose Kogl's laboratory in Utrecht for his postdoctoral year (although transparently the recent isolation of biotin would have been an attraction); (4) most perplexing, as I can find no record whatever on it, what work he did at Utrecht: there does not seem to be any publication on the record; and (5) how he came to the attention of Beadle at Stanford - on this point I would suspect that you must have played at least some role, at minimum the writing of a letter of recommendation which would certainly be a most revealing document if it could be located.

I certainly do not want this list to preclude your thinking about other issues that would be interesting to investigate in a biographical memoir.

I would be glad to honor any confidences that you wish to specify for material that might be ~~assembled~~ for an archive as distinguished from that available for contemporary reference.

Dr. Fred, I guess I was in too junior a position when I first came to Wisconsin to be able to establish very much of a personal relationship with you. I do recognize the stamp of your leadership in the development of experimental science at the university. It takes some hard knocks in one's own career to understand that such things hardly happen spontaneously, and so I am glad to have this opportunity to acknowledge how far my own progress has depended on the foresight and leadership that you had exerted for your department and then for the university.

Sincerely yours,

Joshua Lederberg
Professor of Genetics

JL/rr
Enclosure